FEB 24 1900

NEXT MEETING NEW YORK AND CHICAGO, FEB. 28, 1900.

Entered as matter of the second class at the New York, N. Y. Post Office, November 26th, 1890.

VOLUME XVI.
NUMBER 12. DECEMBER, 1899. Annual Subscription $5.00
Single Copies - - .50

TRANSACTIONS

OF THE

AMERICAN INSTITUTE

OF

ELECTRICAL ENGINEERS.

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PUBLISHED MONTHLY BY THE INSTITUTE, UNDER THE SUPERVISION OF THE COMMITTEE ON PAPERS AND MEETINGS.

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26 CORTLANDT STREET. NEW YORK CITY.
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AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

New York, November 22d, 1899.

The 137th meeting of the INSTITUTE was held this date at 12 West 31st Street, and was called to order by President Kennelly at 8.15 P. M.

THE SECRETARY:—At the meeting of Council held this afternoon the following associate members were elected:

BAUGHER, E. C.	Engineer of Construction, Westinghouse Elec. & M'f'g. Co., 120 Broadway, New York.	R. D. Mershon. F. N. Waterman. Chas. F. Scott.
BAUM, FRANK GEORGE	Assistant to Chief Engineer, (Dr. F.A.C. Perrine) Standard Elec. Co., of Cal., 23 Nevada Block, San Francisco, Cal.	F. A. C. Perrine. F. V. T. Lee. Clem A. Copeland
DOWNES, LOUIS W.	Vice-President and General Manager, The D. & W. Fuse Co., Providence, R.I.	F. V. Henshaw. W.C. Woodward. Gano S. Dunn.
EDWARDS, CLIFTON V.	Attorney-at-Law and Solicitor of Patents, 220 Broadway; residence, 114 W. 114th St, New York.	E. C. Davidson. F. W. Roller. R. T. Lozier.
GIBSON, GEO. H.	Assistant Editor, *Engineering News*, 220 Broadway; residence, 209 E. 14th St., N. Y. City	P.B. Delany. Chas. F. Scott. John J. Swann
GRANT, LOUIS T.	General Manager, The Hilo Electric Light Co., Box 160, Hilo, H. I.	F. F. Barbour. J. A. Lighthipe. F. A. C. Perrine.
HANSON, ARTHUR JAMES	Lawrence & Hanson, 3 Wynyard St.; residence, Drunnmoyne, Sydney, N. S. W.	J.S. Fitzmaurice. G. J. Fischer. R. W. Pope.
INSULL, MARTIN J.	2d Vice-President and General Manager, General Incandescent Arc Light Co.; residence, 262 W. 83d St., New York City.	T. C. Martin. W. D. Weaver. J. W. Lieb, Jr.
JACKSON, E. D.	Engineer, Youngstown Telephone Co., Youngstown, O.	W.E. Goldsborough R. W. Pope. H. B. Smith.

LAYMAN, W. O.	Assistant Manager and Treasurer, Wagner Electric M'f'g Co., 2017 Jackson St., St. Louis, Mo.	R. B. Owens. W. E. Goldsborough D. C. Jackson.
LUNDIE, JOHN	Consulting Engineer, 52 Broadway, New York City.	F. J. Sprague. Louis Duncan. C. T. Hutchinson.
PENDELL, CHAS WILLIAM,	Post-Graduate Student, Mass. Institute of Technology, 207 W. Newton St., Boston. Mass.	Chas. R. Cross. Wm. L. Smith. Wm. L. Puffer.
RAUB, CHAS. B.	Electrical Engineer, Engineer Corps, Newport, R. I.; residence, New London, Conn.	W. R. C. Corson. H. E. Heath. T. C. Martin.
ROBERTS, ALLEN DAVIDSON	Electrical Inspector, City Council, Kingston. Jamaica.	S. Dana Greene. T. C. Martin. E. E. Boyer.
RUSSELL, H. A.	Sales Agent, General Electric Co., residence, 302 Laurel St., San Francisco, Cala.	J. A. Lighthipe. F. F. Barbour. F. A. C. Perrine.
SCHWEDTMANN, FERDINAND	General Superintendent, Wagner Electric M'f'g Co., 2017 Locust St., St. Louis, Mo.	R. B. Owens. W. E. Goldsborough D. C. Jackson.
SCUDDER, HEWLETT, JR.,	Assistant to Prof. Henry M. Howe, residence, 21 E. 22d St., New York City.	F. B. Crocker. G. F. Sever. R. W. Pope.
STOUT, JOSEPH SUYDAM, JR.,	Inspector, Edison Electric Illuminating Co., residence, 35 East 67th St., New York City.	Leonard Waldo. A. R. Ledoux. C. T. Hutchinson.
WHITNEY, CLINTON EUGENE	Draughtsman, with Geo. T. Hanchett; residence, 61 W. 114th St., New York City.	Geo. T. Hanchett T. C. Martin. W. D. Weaver.
WILKES, C. M.	Engineer, D. H. Burnham & Co., 1142 The Rookery, Chicago, Ill.	B. J. Arnold. H. A. Foster. R. B. Owens.

Total 20.

The following associate members were transferred to membership:

Approved by Board of Examiners September 8th, 1899.

CUMMINGS C. CHESNEY,	Chief Electrical Engineer, Stanley Electric Mfg. Co. Pittsfield, Mass.
CAPT. ACHILLES DE KHOTINSKY,	Late Chief Electrician and Torpedo Officer, Imperial Russian Navy, Northern Electric Co., Madison, Wis.

THE PRESIDENT:—The first business of the evening will be the reading of a paper entitled "Test of a 300-Kilowatt Direct-Connected Railway Unit at Different Loads," by Edward J. Willis. Mr. Willis not being here, we will have the pleasure of hearing this paper from Prof. Sever who has kindly consented to read it for us.

[See page 504, October issue.]

Discussion.

The President:—The paper before the Institute is a brief summary, as you have heard, of a test of a 300-kilowatt direct-connected railway unit, commencing with the steam consumption and terminating with the output at generator terminals. The time was when the dynamo was a thing very distant from the engine; many feet perhaps of countershafting and belting separated the two. As the dynamo got bigger and more pretentious, the belting and the countershafting shortened, and now with the dynamo at the size we find it in common practice, the distance between the two has so far diminished that the two form parts of one whole machine, and it is becoming more important to study the unit and the machine as a whole. The paper is therefore interesting from that standpoint and a discussion of the paper is now in order.

Mr. Geo. F. Sever:—I would say in connection with this paper, that it seems as if a very important feature had been omitted from the test, and that is the temperature of the dynamo during the run. I fail to see a record of it in the table. It seems to me that this feature is just as important and perhaps more so than almost any other of the items included in the table. I think, that in presenting this matter to the Institute as a record, such an important measurement as the temperature ought to be stated.

Mr. Willis: [Communicated] The temperature of the dynamo during the run would have been given, had not the thermometer used for this purpose been accidentally broken at the end of the second run, and the location of the plant rendered it impossible to replace it in time for the test.

Mr. Geo. Hill:—In first glancing over the table it appeared that in the section labelled "calculated results" that an error had been made. Further examination shows that the results as stated are substantially correct. The points which seem to be brought out by the paper are:

1st.—That in all direct-connected work where the load carried by the generator violently fluctuates, it is economy to design the engine so that it can safely develop the maximum horse-power with a late cut-off, the engine cutting off at about a quarter stroke for from 60 to 70 per cent. of rated capacity.

2nd.—The generator has rather a flat efficiency curve, but is certainly not generously proportioned if, as the author states, it became too warm after running for an hour at 500 amperes. Under fluctuating load this perhaps is no objection, if the saving in material carries with it a saving in price, but as between two generators of the same price it would be a disadvantage.

3rd.—While it is economy to use an engine of less rated capacity than the generator would ordinarily demand for constant load, is is not economy to use the boiler of the same rated

horse-power as the engine, the efficiency of the boiler when beyond its rated capacity showing a noticeable falling off. It may not be amiss to call attention to the fact that the setting of the valve of the low pressure cylinder as shown by the indicator cards is bad, the cut-off being much earlier at the right hand end than at the left hand end in each case.

MR. WILLIS: (Communicated.) The generator had been running continuously just previous to the testing and was comfortably warm when the test was started which had something to do with the fact of its heating up as stated. The writer has little doubt that the generator would have stood the higher load, but preferred to err on the safe side and not submit the generator to unnecessary temperatures.

Mr. Hill is particularly correct in his remarks about the boiler capacity not being sufficient. The marked falling off of the efficiency of the boilers at heavy loads, to which Mr. Hill very thoughtfully calls attention, was due to a number of the baffle bricks having fallen in. This did not show at the lower loads, but caused the high draft flue temperature and low evaporation shown in the test for the higher loads. The test led to a very careful examination of the boilers for such troubles, which resulted in a thorough renewal of both bridge and baffle walls. After these repairs the boilers showed up considerably better when heavily loaded.

MR. C. O. MAILLOUX:—I think it would be desirable in future in tabulating results of this kind to include the friction tests of the engine, so that one might have a clue as to the mechanical efficiency of the engine itself. I am led to this statement by noticing the last column of the calculated results which give indicated horse-power per kilowatt. Of course here we are dealing with a quantity which has no reference whatever to the coal consumption. It is purely a ratio of the amount of indicated horse-power to the net electrical output, in kilowatts. Now it would seem that for an engine of that size the values ought to be very much lower, if the engine or the dynamo did not have excessive friction. I have always made it a practice in making tests of this kind to notice the indicated horse-power per kilowatt, because it is a quantity that is really of great interest, since it takes into account not only the efficiency of the dynamo but that of the engine as well, and an engine which would have a very low friction load would of course materially help the efficiency of the unit as a whole. In a unit of this size I would not expect to find a value higher than about 150 or 155. I find that in a well-arranged, direct-connected unit even at small sizes, from 50 to 150 or 250 K. W.—it very seldom exceeds 1.6 at full load. In this case even at 500 amperes, which is considered very nearly full load, it was still 1.7., which, it seems to me, is a very high figure for a unit of this size. One might expect such a value as that with a dynamo of 25 K. W. direct-connected, but

scarcely with one of 300 K. W. For this reason, it would have been interesting to give the friction cards showing the friction load of the engine with brushes on and with brushes off, so that one might have some clue as to where some of this energy has been lost, and over what portions of the generating set it is distributed.

MR. E. J. WILLIS [Communicated.]: Mr. Mailloux is correct in his statement that the indicated horse-power per K. W., as given in the test, is high. As stated in the paper the author has not offered the paper as one showing especially high economy, but as one giving reliable results obtained under usual running conditions. The friction load of engine with brushes down was 51 horse-power. The friction load with brushes up was not determined.

THE PRESIDENT:—Is there any further discussion of this paper? If not, we will pass to the second item on the programme for the evening, which is a topical discussion on the "Possibilities of Wireless Telegraphy," and we will call upon Prof. Fessenden to open that discussion.

A Topical Discussion at the 137th Meeting of the American Institute of Electrical Engineers, New York, November 22nd 1899. President Kennelly in the Chair, Chicago, Local Honorary Secretary Pierce in the Chair.

THE POSSIBILITIES OF WIRELESS TELEGRAPHY.

[A Topical Discussion.]

Prof. Reginald A. Fessenden:—Whilst there are many advantages in living in a city in which there is a widespread and intelligent interest taken in scientific work, there is this disadvantage, that when any discovery of a striking nature is made, the professor's friends and the directors of the institution he is connected with expect him to immediately lay aside his own work; work which he may personally consider of much greater importance than the novelty of the hour, and to assist in the development of the new discovery.

Having thus been forced, some years ago, into X-ray work, with much loss of time and very little results to show for it, I considered myself proof against the seductions of liquid air and wireless telegraphy. Consequently when, having suggested to one of the editors of the *New York Herald* that they report the international yacht race by the new method, I was invited by them in December, 1898, to undertake the work myself, I declined and put them in communication with Signor Marconi.

It was found later, however, that there were some exceedingly interesting questions, which had not been solved, in this connection. In none of the work hitherto done had any *exact measurements* of the quantities involved been made. Consequently many points were still in doubt. The theory of electromagnetic waves had been well and thoroughly covered by Heaviside. But if the theory, as put forward by him, is correct, it is extremely difficult to account for Marconi's law, that the distance of transmission, other things being equal, varied as the product of the heights of the sending and receiving wires. And yet the great experimental ability shown by Signor Marconi in his admirable work rendered it necessary to suppose that in forming our theory we had omitted some consideration which, known to Marconi and his assistants, were as yet unknown to us.

Unfortunately the receiver in general use, *i. e.*, the coherer, is very ill adapted to quantitative measurements. A simple and

very sensitive form of receiver having having suggested itself, a few experiments were made in June, but the matter would have been dropped had it not been that my former assistant and present colleague, Professor Kintner, with the greatest of kindness offered to help me, and with the aid of his invaluable constructive and experimental ability, it has been found that it will be possible to carry the work to a successful conclusion, though as yet but a few of the points which it is proposed to investigate have been covered, and these not as yet fully.

As we have already had one paper this evening and there are some other speakers to follow, I will only touch on one or two points which may be of interest, and on them as briefly as possible. I shall leave the discussion of the question of long distances to others, and shall give, as my contribution to the discussion, an account of the first quantitative measurements which have been made in this line, and some of the results obtained.

I will first show a model which is supposed to represent the form of the waves which are concerned in the phenomena, so far as we can tell from the latest theories, (or rather from the theories on the subject, because some of them are not of recent date. Heaviside showed a number of years ago what the general shape of these waves ought to be.) I will then show and describe some forms of receivers which I have designed, and which have shown themselves especially valuable for making quantitative measurements. Of course Marconi's apparatus is peculiarly adapted for practical work; but it is not so well adapted for giving exact measurements of the amount of energy or voltage. Moreover, it is not nearly so sensitive as the receivers I shall show. I will then state briefly how my own experiments have agreed with the theory and take up one or two practical points which have developed in the working.

The piece of wire and pasteboard here shown [Fig. 1,] is supposed to represent the waves: A is supposed to be the sending apparatus; the pole which is connected to the induction coil. When the break of the induction coil occurs the vertical conductor is charged, and the electrostatic lines, according to Heaviside, come out and strike the ground at B. I don't remember exactly into what detail Heaviside goes. He gives the general shape of it. The electrostatic waves come out all around, of course; this model being merely a slice out of the wave front. The model represents the condition of affairs when, after some waves have been emitted, the tension is a maximum. As soon as the spark passes between the two terminals of the induction coil there is a conducting path formed through the heated air to ground. The ends of the lines can now slide down the conducting wire to ground and form a sort of a half hoop, as shown at C,—and travel outward. The whole thing, for a cylinder, is worked out in Prof. J. J. Thomson's supplement to Maxwell.

As it goes out, as you will see from Fig. 1, the rear ends of the half hoops bend more and more in, as at D, until finally when you get some distance away from the exciting source the fronts and backs of the waves are parallel to one another; and form parts of concentric spheres whose center is the originating point A.

This kind of wave was first experimented with by Lodge, so far as I know. It differs from a true light wave in this, that the two ends of the wave slide over the conductor. If you can imagine the pasteboard strip, E, a mirror, and suppose that the other half of the wave was underneath it at X. as if it were its reflection in the mirror, you then get the full wave, such as you get from light, or from the regular Hertz oscillator. But, as I say, this differs from it, in that the ends of the electrostatic lines slide along the surface of the conductor as they go

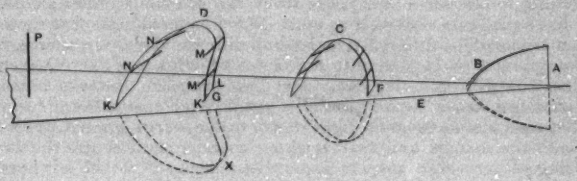

Fig. 1.

out. The wave-length is approximately four times the height of the exciting pole. The model is made to scale. The wave-length from F to G being four times the height of A. Consequently, if you double the height of your pole you double the length of the waves.

K. L, represent the electrostatic lines. M. N. represent the magnetic lines, sticking out at alternate ends, to indicate their direction. When a wave in going along strikes a conductor, such for instance as the wire P, the magnetic lines cut it and in cutting it they create a voltage in it. This voltage, incapable of being supported in the conductor itself, concentrates itself down at the bottom of the pole across the gap of Marconi's receiver and sparks across, reducing the silver sulphide or oxide, and when the current has once passed, the insulation being destroyed, the relay works. The periodicity of these waves is pretty high. Traveling at the same velocity as light, we find that

if we have a 150-foot pole the periodicity comes out about three millions per second. If we take a pole such as would be used to send signals across the ocean, a thousand feet high, we find that the periodicity is 400,000 per second.

Another point in which these waves have a great advantage over the ordinary light waves is that they follow the surface of the conductor. If, for instance, we have a hill, the waves slide up and over that hill. It is for this reason that I think we can never hope to obtain anything from direct Hertz waves, thrown by a reflector, because those will not work that way; they go straight out, and the only chance that they have is through striking the water and being cut off or reflected a little bit and then sliding along in the same way as the Lodge waves.

Since these waves travel over the surfaces, the surface must be a conductor, and this naturally has considerable to do with the strength of the waves received. On land, the waves are frittered out very much more quickly than they are on water. For sea water and land the proportion seems to be about three to one, so far as one can judge from Mr. Marconi's experiments. I have made none myself as yet. But I have noticed that there is a tremendous difference on land on different days. On a muggy day you get perhaps five to ten times the throw of the receiver that you will on a dry day. Ice does not seem to make much difference, so far as we can tell; possibly for the reason that in that case the wave goes right down to the warm ground underneath the surface and travels along there, and of course the ice being of very high specific inductive capacity it would not hurt matters very much.

It is interesting to calculate what the effect of distance ought to be. In the present case you see that at twice the distance you have the same number of lines on the wave front; but in order to get the whole wave front, your pole has got to be twice as high. In other words, to get the same voltage piled up on the receiving conductor over at R you must have a pole just twice as high as it has to be at C. Or, for a given height of pole the voltage available decreases directly as the distance, and with a receiver like Marconi's which depends mostly on the voltage—the coherer would have to be twice as sensitive to work at twice the distance, the pole being of the same height, or in other words at double the distance a given coherer should be worked with double the spark-length at the sending end. The energy, however, is only a quarter as much for a given height of pole. At the receiving end, if you double the height of the pole you would get double the voltage on it. At the sending end, if you double the height of the pole *and at the same time double the length of the spark*, then you will get a wave exactly the same, except that all its linear dimensions are doubled, and you will get double the voltage at the far end. So that theory indicates

that Marconi's rule should be modified to this: "If you double the height of the receiving pole and at the same time double the height of the sending pole and also double the length of the spark, the distance at which you can receive should be four times as great." I cannot find that Marconi's rule ought to hold *unless you also double the voltage of the spark*, because the effect of doubling the height of the sending wire ought not theoretically to have nearly as much effect as doubling the height of the receiving wire.[1]

The energy, however, comes out somewhat differently. If you double the height of the pole at the far end, you get double the voltage. If you double the height of the pole here, and at the same time double the voltage, you will get four times the energy near the origin in a given slice. But if your receiving pole is over at P and the magnetic lines come along and strike it, the conductor gathers in a certain fraction of the width of the wave and this fraction is approximately, as near as I can figure it, about one-seventh of the wave length, the amount of energy that the conductor scoops out of the wave front depending on the length of the wave itself. Consequently, when you double the height of the sending pole and double the wave length you scoop out a bigger area, twice as much, doubling the energy, but not the voltage received. Take your receiving pole, its height here gives you voltage. Then lay your sending pole crosswise, like this, so as to get a rectangle; the product of these two is proportional to the total amount of *energy* per wave. If at the same time you have doubled your voltage you would get eight times the energy, but you only have half as many waves per second. So that using a receiver which works by energy and not by voltage, the distance to be sent ought to vary as the product of the heights of the poles if the spark-length is proportional to the height of the sending pole.

I will now describe some of the instruments that I have made. The first instrument is shown in Fig. 2. It is heavier than necessary, but is nevertheless quite sensitive. The ring A is a bit of No. 26 wire hung on a quartz fibre with a mirror M attached. B is the collecting wire. The magnetic lines strike this and make a voltage, and the resultant current goes around this coil C, around through the coil D and then to ground. The closed ring is at 45° to the coils C and D. The principle on which it works was first discovered by Elihu Thomson. The current coming in through C and D makes an alternat-

1. At the close of the discussion, one of the officers who was engaged on the recent tests of the Marconi system for the Navy here, informed me that this was the case, *i. e.*, that it was found in practice that if two vessels had wires of different lengths, communication should be carried over a longer distance when the vessel with the higher pole was the receiver than when it was the sender. This is a very satisfactory corroboration of the theory.

ing current field. That alternating current field sets up a voltage in the ring A. The voltage in the ring makes a current in the ring, and the current in the ring reacts on the original magnetic field. The consequence is that the ring tends to turn. It is a very nice galvanometer to work with. You have a quartz fibre. Your zero always stays the same. There is nothing magnetic about it. For the best results it should be connected with a condenser right across it. Of course, the reason of that is obvious. You generate a voltage in it, and if you have got a condenser as at F, then for a given voltage you can get a considerable current in the coils, that current simply depending on the resistance of the coils, and you do not fritter any energy away in current up and down in the receiving wire; whereas if you

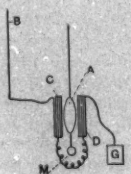

Fig. 2.

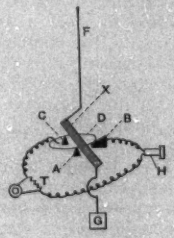

Fig. 3.

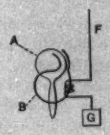

Fig. 4.

put a condenser in series with the instrument, your whole current flows through the receiving wire, tapering off, of course, as it goes up, and you lose a great deal of energy in resistance. This is the first and simplest form, and is the form that is used mostly. It works very nicely.

Fig. 3 is a form which I have tried a little but have not had time to work out very thoroughly, A is a little block with silver wire on it. B is a v-shaped piece of carbon. C is another block with wire on it. D is a closed silver ring which is laid right on top of A, B and C, so that it almost balances on A and C. F is the collecting wire and the current comes down from the pole and goes through the coil X as before, and out again to ground, with a condenser across X as before. You put this ring D on so that it lightly rests on the microphonic contact B. T is a source of A. C. voltage, very low, and H is a telephone. When the current passes through the coil X, it presses the ring down and the previous slight noise in the telephone is made louder, and on the duration of loud noise depends whether the signal is

a dot or a dash. This seems to be pretty nearly as sensitive as this other instrument here.

The third instrument which is more sensitive still, but which I have not had time to work very much on, is shown in Fig. 4. This looks, at first, a little like a single-phase motor. A is a wire ring made in a figure 8 form. B is a piece of iron wire bent into the shape shown. As a matter of fact, in the instrument made, this wire went three or four times through A. F is the collecting wire. The magnetic flux is linked with a part of the ring, and the ring itself has one edge in the magnetic field. The result is, a voltage is induced in the ring, and the current flowing around in the ring acts on the air-gap flux and the ring tends to twist. It is quite sensitive.

So far as the results obtained, I would say that they seem fairly well to agree with the theory, but as yet not so well as I could have hoped. For instance, doubling the spark-length, you ought to get four times the deflection; whereas I did not get that, though, perhaps, this was due to a weaker stream of sparks. Here is a record of one set of observations made:—

When the spark-length was 40, the deflection was 60.
When the spark-length was 32, the deflection was 62.
When the spark-length was 24, the deflection was 33.
When the spark-length was 21, the deflection was 30.
When the spark-length was 13, the deflection was 10.
When the spark-length was 9, the deflection was 6.
When the spark-length was 5, the deflection was 3.

And so on down. The deflection does not quite vary as the square of the spark length, and it ought to.

The time during which the spark stream is made, seems to influence the deflection very nearly as it ought to. For instance, one result was: Time during which the spark was occurring, 2 seconds, deflection was 20. Time 10 seconds, deflection 94.

The results obtained are merely preliminary, and before giving them in detail, I want to repeat them under better conditions. To get accurate results you must have a good ground. You must go for the best results to some place near salt water. The ground seems to depend a good deal on the state of the weather, as I say. Sometimes running a wire direct from one station to the other will not improve the ground more than 20 or 30 per cent. At other times, the ground will go away back on you. And it does not seem to depend on contacts, because all the contacts made were amalgamated with mercury.

The results are not always consistent. Sometimes you get five results the same. Then suddenly, owing to something in the sparks you will get a kick five or ten times as big. One curious thing I might mention was that we got some very hard kicks while we were working. Finally we traced the cause up to the fact that about three or four hundred yards away there

was an electric street railroad, and as it went around a certain corner there was an arc owing to the trolley sparking over a contact. Ever time that arc occurred we got a tremendous kick. The trolley itself seems to act as a sending pole.

The following are the points which have been or will be investigated:

1. Effect of change of spark-length.
2. Effect of different kinds of break.
3. Effect of condensers in sending wire, in shunt to air-gap.
4. " " in series.
5. " " " with receiving wire.
6. " " in shunt to receiver.
7. " varying capacity of sending wire.
8. " " " receiving.
9. " " heights of sending and receiving wires.
10. Efficiency of different portions of sending and receiving wires.
11. Efficiency of different kinds of grounds, especially of what may be called the crow-foot ground.
12. Efficiency of different forms of receivers.

Some work has been done in nearly all of these lines, but it has been found necessary to turn all our attention to the question of a reliable ground, and when this is settled we shall return to the other questions.

I have to express my sincere thanks to Professor Kintner for his invaluable assistance, without which it would have been impossible for me to have taken up the subject.

Mr. W. J. Clark: I don't know that I have very much to say this evening on the subject of wireless telegraphy after listening to the very interesting account of Prof. Fessenden's experiments. Perhaps the best thing I can do is to give you my experience. I suppose you all know that during the yacht races I had the pleasure of being associated with Mr. Marconi in reporting the races for the *New York Herald*. During that time Mr. Marconi had a receiver on the Highlands at Navesink connected with a wire about 115 feet in height. He also had a receiver on the cable boat *Mackay-Bennett* connected to a wire I think about 115 feet in height. I had a transmitter and receiver on the *Grand Duchesse*, 115 feet of wire, and a Thomson induction coil for my transmitter. I used ¾ inch balls which are always furnished with the Thomson Roentgen ray apparatus for my oscillator. The balls were in very bad shape. They had been used for a long time without either being cleaned or polished, and in spite of this fact we had no trouble whatever in sending the signals in to the cable boat and also in to Navesink station. Mr. Marconi was on the *Ponce*, and had no trouble whatever in reading our signals. I have constructed two different styles of receivers. One is a very expensive instrument, provided with a great many adjustments for con-

venience. Another is a smaller instrument intended principally for demonstration work. I made a test with my expensive instrument, and then with the other one, and I found that the simpler instrument gave me much better results over the long distance, and some days I was receiving from Mr. Marconi at the rate of between 10 and 15 words per minute from a distance of fifteen miles. I have noticed one thing in experimenting, and that is, that it is necessary to have the capacity of the two wires equal as nearly as possible. During the recent electrical exhibition I was asked to send the Governor's message from the Pulitzer Building to Madison Square Garden. I was only given about one or two days at the outside to get ready, and of course that meant a great deal of work in a very short time. We placed about 260 feet of wire on the Pulitzer Building and about the same amount on Madison Square Garden, placing the transmitter in the Pulitzer Building, and the receiver at the Garden. We found it impossible to get any results whatever. We could not get the slightest sound from our receiver, although it was a very sensitive instrument, and the transmitter was a very powerful one. Our wire was rubber covered, and thoroughly insulated from each building. The way we insulated these wires was by hanging them from a rod of hard rubber about two feet in length and about an inch in diameter. We used a *large* rubber rod in order that it might be able to resist the strain upon it. At the lower end of the wire we used a similar rod, and then took a tap from our wire and led it in through a window on whichever floor we happened to have our instrument. After the exhibition was over, I made a test between Madison Square Garden and our laboratory, which is not over a block and a half distant. Using about 260 feet of wire on the Garden and about 60 feet at the laboratory, I could get no results whatever, no matter which end I placed the transmitter on, and I was rather puzzled. I found, however, in conducting some other experiments on the bay, and also in the city, that it was absolutely necessary to have the two wires of about the same capacity. We have been able to transmit altogether, aside from what we did at the yacht races, to a distance of about seven miles, and our signals were very clear, indeed. However, I must accord to Mr. Marconi the credit of having by far the best apparatus, better than any I have seen in America, including my own. His receiver is extremely non-sensitive. The receiver which Mr. Marconi was using at the races was a receiver which, under ordinary conditions, that is, under the conditions under which I have experimented, would not receive a message from my transmitter at a distance of more than a mile. Now, Mr. Marconi tells me, at least he hints to me, that the way he accomplishes this result is by measuring the capacity of each wire, and having them precisely alike.

I have recognized for a long time that the coherer was a very

troublesome piece of apparatus, and that it was something which we should try to get away from as much as possible. Consequently, I have been experimenting for some little time on a new kind of receiver. I should have said that when Mr. Marconi was here, although his instrument was receiving from his own transmitter at the rate of but from 18 to 22 words per minute, he said to me that he thought his receiver had about reached the limit of the transmitting operator's speed. I would have liked to have the opportunity of trying to transmit to his receiver using our American Morse code, but that unfortunately was impossible, because Mr. Marconi's assistant could not decipher that code. Now, I I have found that by using a new arrangement in the receiver and doing away with the coherer entirely, that we are able to transmit at a very much greater speed; in fact, that we are able to transmit as fast as we can operate the key. I have only experimented with this very recently, only during the last few days; that is, I have only brought it to a state anywhere near perfection during the last few days.

Another peculiarity I have observed is this: that in the transmitter we get the best results by far, when the secondary of our coil is wound with the finest possible wire. You know when the X-rays first came out, everybody said that we must wind our secondaries with a much coarser wire than they were wound with before. The consequence was that nearly everybody was provided with induction coils with coarse secondaries, giving apparently a heavy spark. Of course we all experimented with those coils because they were convenient; but I happened to have access to one of the old style coils with very fine wire on the secondary, No. 40, I think, and I found the results were very much superior to what I was able to get with the coils wound with a heavy secondary, although the coil with the fine secondary was a very much smaller one, very much shorter spark-length than the coil with the coarse secondary. I think from this, that there are indications of the fact that the balls of the oscillator are not necessary at all. You will remember that when Mr. Marconi first began to experiment, he used balls of four inches in diameter, and stated in some of his publications at that time that the larger the balls to a certain extent, the better the result. Now for 110 miles transmission he is using balls one inch in diameter, and if I had my apparatus here to-night, I could show you that it is quite possible to transmit signals across this room with the balls so far separated that you do not get any discharge at all, and I could also prove to you conclusively that waves did not emanate from the spark at the interrupter. I think from all this, as I said before, that the indications are that we will get along without the balls at all or without any spark. When Mr. Marconi was here he had the kindness to visit our laboratory and I was showing him one of our small outfits and showing him how it would operate by opening a motor switch on the wall

with 220 volts and a very small current, and Mr. Marconi expressed his very great surprise that a spark of that kind would work any receiver, because he claimed it was not a static spark.

Another thing I have noticed is this, that for instance if we have a receiver on this table, and a transmitter on the other table, a very small one, say giving a half-inch spark, if we separate the balls to the full distance of one-half inch, we will not get anything like as good results as if we bring them to about one-sixteenth of an inch, and in a five-mile transmission I have found much better results from a spark half an inch in length than from one inch in length; but in each case I was using a coil of 10-inch spark length, so that the distance of the balls from each other would not interfere with the continuity of the spark.

I do not know that I have anything further to say; but before sitting down I would like to ask Prof. Fessenden what kind of spark he has been using in his experiments and what difference he has found, if any, between the heavy spark and the light spark, so to speak.

PROF. FESSENDEN:—I used an ordinary induction coil for the work. Part of the time I used it with the ordinary break, and part of the time with a Wehnelt break. I did not find very much difference between the two. I found a one one-hundredth of an inch spark to work very well at about 200 yards with 15 ft. poles, and it did not seem to make very much difference what the kind of spark was. I did notice however that when we got a flaming spark, the results were not quite so good. There is always a little risk in making experiments with the sender too near the receiving instrument, because electro-magnetic effects are apt to come in. I noticed this when working at first from one end of the laboratory to the other. The core of the induction coil, I found, would make sufficient stray magnetism to work the thing.

I will say, as regards the sensitiveness of this particular instrument, that it would seem as if a mechanical device ought to be better than the coherer, for this reason—we can make a galvanometer which will work with a current of about 10^{-11} amperes and a resistance of about 100 ohms or less. That makes about 10^{-20} watts or 10^{-13} ergs per second. I do not think that you can make a coherer work much under a fraction of a volt. What is the smallest that you have succeeded with, Mr. Clark?

MR. CLARK:—I have not experimented in that line at all.

PROF. FESSENDEN:—I do not think you can make it to work very much under a volt; that is of excessive voltage above the regular voltage of the battery, and in that case, roughly taking the quantity of electricity as equal to the smallest amount possible, that is to say, enough to charge it electrostatically, which would make its capacity about unity, we find you would get about five millionths of an erg. Now reducing ten seconds period down to a tenth of a second, you find the galvanometer should

be about ten thousand times as sensitive. Of course you won't get this; but it seems to show there is some hope that way[1]

Dr. M. I. Pupin:—I would like to ask Mr. Clark why he did not clean the balls. When he said he worked with dirty balls, it seemed to me it was a very small matter to clean the balls. Why didn't he clean them? Was it on purpose or accidentally?

Mr. Clark:—I did not clean them because Mr. Marconi said to me that he found better results at the present time with rough balls than with smooth ones, and that rather tended to confirm the results of some of my own experiments.

If I may be permitted I would like to ask one more question. I do not profess to be a theoretical man. I am working entirely on practical lines, and perhaps some of you this evening, perhaps Prof. Fessenden, can set me straight on one or two points. Prof. Fessenden mentioned that he was able to get transmission across the room by means of the core of the coil. My question to him may not seem a very bright one, and if so I ask it in ignorance. Why is it then that if we take a very powerful magnet and bring it in the neighborhood of the coherer and move it around all that we please, we cannot affect the coherer in any way.

Prof. Fessenden:—I think that the reason of that would be that when the interrupter breaks the circuit the iron demagnetizes in about the ten-thousandth part of a second, or something of that sort, and you would have to agitate yourself very vigorously in order to get equal results.

Dr. Pupin:—What was the spark-length that Mr. Marconi had?

Mr. Clark:—One inch—about two centimetres.

Mr. F. V. Henshaw:—I would like to be enlightened a little bit on the spark-gap question. I had an impression that we had some good rules to go by in regard to proper height of pole and length of spark-gaps, etc.; that if you wish to transmit a long way, you should have a long spark, and if you want to transmit a short way, you should have a short spark. Now it appears that the length of the spark doesn't make any difference, and later on we learn that we can do it all right with no spark at all.

1. Note B. The original instrument described can be used for receiving signals up to 5 miles with a 20-foot pole and 5-inch spark, even without the use of condensers. A slight change recently made, has increased the sensitiveness about 40 times, still without the use of condensers or tuning. For those who may wish to experiment in this line, I would say, that the following dimensions will give an instrument much more sensitive than the coherer, and capable of use over long distances. Coils, two in number, 400 turns of No. 32 wire, boiled in paraffin. Coils $\frac{1}{4}$ inch inside diameter and $\frac{1}{4}$ inch long. Movable ring, No. 26 wire, $\frac{1}{4}$ inch diameter, mirror $\frac{1}{4}$ inch diameter.

Where great sensitiveness is required, use smaller coils and ring of silver with a scrap of silvered glass, very thin and $\frac{1}{16}$ inch diameter fastened to ring. Observe motion by reflected spot of light from glass. A silvered film of mica or celluloid might be used where great dampening is desired.

It may be of interest to mention that in sending signals across $1\frac{1}{2}$ miles of city buildings, diagonally across blocks, the signals were only about 10 per cent. of what they would have been over free space.

If there are any quantitative results with regard to the ratio of the spark-length to the other factors, I think it would be very interesting to have them.

PROF. FESSENDEN:—I have just given my measurements on the relation between the length of spark and the effect at the receiving end. I pointed out that the effect at the receiving end did not quite vary as the square of the spark-length. So that in doubling the spark-length you do not get quite four times the effect, as you should.

MR. HENSHAW:—Then there must have been something in Mr. Clark's results that masked that. Didn't I understand you to say that you got better results with a one-sixteenth inch spark than with a long spark?

MR. CLARK:—Yes, for a short distance, and Prof. Fessenden explained how that occurred. He has experimented to a greater extent in that line than I have. But Mr. Marconi has transmitted 110 miles with a one-inch spark, and he uses a one-inch spark from five to ten miles, just the same. In fact, he has adopted a spark of that length as a standard.

MR. C. E. DUNN:—Mr. Clark, I believe, said a few moments ago that he found that he got results without any spark at all. I would like to ask him if he had any idea from what the effect arose.

MR. CLARK:—I do not claim to give any reason for it at all. I simply state the fact. Prof. Fessenden, as you have heard, states that he thinks it was due to the magnetic influence from the core of the coil. But I have tried the action of a very powerful magnet in the neighborhood of a coherer, and making and breaking the circuit of that magnet with a vibrator, placed at a safe distance, I have found no results. But in case any of you may be experimenting with the interrupter of your coil in a vacuum, I want to tell you something that happened at the exhibition over in Boston. We were blowing up a boat every day, four times a day, by wireless telegraphy from across the hall, and we found, to our astonishment, that at about two o'clock each day something interfered with us, and we consequently had to blow up the boat a few minutes before two. Now, Mr. D. McFarlan Moore had his artificial daylight down in the basement, several hundred feet away from us, and we found that he started up about two o'clock, and after a little experimenting we found that we got these waves from Mr. Moore's apparatus. You all know that his interrupter is in a vacuum, and we could not find any sparking or anything that we thought would influence our receiver, except the sparking of the vacuum; so after that we claimed to blow the boat up by artificial daylight.

DR. PUPIN:—In reference to getting the effect without the spark, I think I was the first to show Mr. Clark that under certain conditions it could be obtained. Mr. Clark, two or three years ago, showed his apparatus to the New York Electrical

Society. I was its president then, and heard Mr. Clark's lecture. I tried the apparatus that Mr. Clark showed, and I saw that it would work whether the interruption of the primary current produced a spark or not. I think Mr. Clark was very much surprised that such was the case. Of course, the distance was the length of a table, and the receiving line was connected to a water pipe, and the transmitting line was connected to the same water pipe, so it was equivalent to having the receiving and transmitting line connected, being one line, of course, and as soon as the primary was broken, of course the transmitting line as well as the receiving line was charged, and that charge produced a spark in the coherer and made it operate. It is, of course, not necessary for a short distance like that to have a spark at all. But, if you take 100 yards distance, I am perfectly sure you could not get any effects at all unless you have a spark.

MR. CLARK:—On the occasion which Dr. Pupin mentions, one wire was connected to the water pipe, and the other to the gas pipe—at least, I supposed that was the case. But I found, to my delight the next morning, that our man had not carried out his instructions, and the transmitter was not connected to a ground at all. At the present time, with the instruments that we are now using, I find no difficulty at all in transmitting 30, 40 or 50 feet with only a perpendicular wire of about eight or ten feet in height, with no ground connection whatever, which of course is very much more interesting. I have also succeeded in directing the movements of a model automobile, quite a large-sized piece of apparatus, weighing about 50 pounds, with no trouble at all.

THE PRESIDENT:—Was that with a spark?

MR. CLARK:—Yes, with a spark.

THE PRESIDENT:—If there is no further discussion, I think that we ought to extend a vote of thanks to Prof. Fessenden and to Mr. Clark for their entertaining descriptions. The subject is an extremely fascinating one, fraught, perhaps, with many possibilities in the future, and the discrepancies which are bound to occur between the evidences of practice and the prophecies of theory are only the more interesting, since they show the great complexity of this subject and the many facts which enter into each case. But, no doubt, as we get more experience on the one hand, as we acquire better apparatus on the other, and as we also get more and more insight into the nature of the phenomena which are involved, we ought to be able to bring theory and practice into a more complete degree of uniformity.

DR. PUPIN:—I was invited to take part in this discussion, and I came to discuss the subject, but I would not carry out my resolution, if I did not think that we have not yet touched upon the possibilities of wireless telegraphy. So far we have had an exchange of personal experiences. These experiences are all very interesting, and I am sure that we are very much obliged to

Prof. Fessenden and to Mr. Clark for their very valuable information. It is extremely important that we should know from actual personal experiences what are the difficulties involved in wireless telegraphy. But the subject of this evening is "*the possibilities of wireless telegraphy*," and I do not think that the AMERICAN INSTITUTE should adjourn without saying something relative to these possibilities. Of course when a man speaks about the possibilities of anything, it is all in the future, and he has poetical license—he can indulge to his heart's content. Now, if you will allow me to indulge a little bit, I will tell you something about what I think might be the possibilities of wireless telegraphy. My ideas on the subject are not an offhand notion. I have thought a great deal about the subject. I teach in a college; am called upon quite frequently by young students to tell them something about wireless telegraphy, and if I go out into society or among business men, and they know that I am a professor at a college, they think that I know all about wireless telegraphy; so they come to me and ask me all about it, and of course I try to give them an intelligent answer. So, whether I want to or not, I have to think a great deal about it. It is in the line of my business. If I make a mistake in my figures, or if I should be a little too sanguine in my expectations, you will be very indulgent with me, because I am speaking about mere possibilities.

One of the possibilities is the tuning of receiving apparatus. The chief difficulty to-day is that if two transmitting stations are working at the same time, the message received at the receiving apparatus is unintelligible, because they interfere with each other. It is, therefore, very desirable that the receiving apparatus should be tuned so that it will respond to one transmitting apparatus only, and to no other. The second important point is that we should extend the distance as much as possible. These are the two important points, and I think the possibilities in those two directions are very great.

Now, if you will allow me to go to the blackboard, I shall try by diagrams to make myself clearer than I might otherwise do. The evening is still young, and we have lots of time; so don't get impatient. In case you want to go, why do so, I won't feel offended.

The transmitting wire as it is constructed to-day consists of a long vertical wire, *a b*, Fig. 5, and a spark gap *a c*, and an earth connection *c*, and the receiving wire is *e d*, the coherer is at *f*, and the local circuit *g f h*, the induction coil A, is also indicated connecting to the two sides *a c* of the spark-gap. I would like here to ask Mr. Clark a question. How many breaks per second did Mr. Marconi employ:

MR. CLARK:—I cannot answer that question definitely. He had a very rapid rate, the most rapid one that I have heard. I can only guide by the ear.

DR. PUPIN :—The fastest mercury break used with induction coils that I have seen, did not go very much over thirty a second.

MR. CLARK :—MR. Marconi said that all he required for dots was one spark between the balls.

DR. PUPIN :—That is not the point, though, that I wanted to know.

PROF. FESSENDEN :—You can call it 500 per minute possibly. When it gives a high pitched note, it would be somewhere around there.

DR. PUPIN :—That is about 8 per second. When you excite two wires like $a\ b$ and $c\ G$ until the potential is high enough to make the spark in the spark-gap, then each spark is followed by a series of oscillations. They are the so-called free oscillations of this wire—the wire $b\ a\ c\ G$. The wave-length will depend

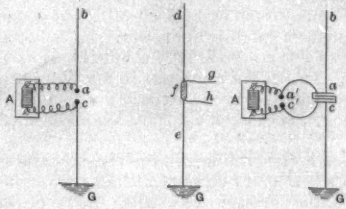

Fig. 5. Fig. 6.

on the nature of the excitation. I do not care to go very deeply into that for two reasons—because I don't know much about it, and secondly, it is not necessary—two very good reasons. I will tell you why it is not necessary, and why I do not know much about it. Take a wire like $b\ G$, the capacity of which is a variable quantity, because the capacity of any element depends on its length and its distance from the earth. As you go up along the wire the capacity of the wire is continually diminishing. The free vibrations of a wire like that, are like the free vibration of a string, the elasticity of which varies from point to point. Now, no man on earth would be able to tell you with any degree of exactness what sort of oscillations you would get there. I don't think you would get a pure harmonic oscillation. There will be an oscillation of all sorts of unrelated frequencies. If we had an ordinary wire where the capacity per unit length is a constant quantity from beginning to end, the problem is very easy; it has been solved; the wave-length that the wire emits is a multiple of the length of the wire. Prof. Fessenden told us that the wave-length is about four times the length of the wire. Now, these oscillations are very much

dampened, for this reason—that you have a great deal of dissipation of energy right in this spark-gap. If you could excite *oscillations in a wire without a spark-gap*, these oscillations would have some damping, but not very much. They would be, so to speak, sonorous oscillations, as when you strike a bell made of fine bell-metal, it continues to ring for a long time after the stroke is delivered. But if you put a finger on the bell and strike it then, or put in some resistance, the sound dies out very rapidly. So that is the state of affairs in this transmitting wire with a spark-gap. You have the high resistance, dissipation of energy, and the oscillation is a very damped one; you have only a very few waves sent out after each spark. Now, when you have a train consisting of very few rapidly diminishing waves, they cannot produce much resonance.

That is the reason why they have not been able to tune their receiving apparatus in England, that is, I think so I don't know anything in this line for certain. I know only one fact, and that is that nobody has been able to tune the receiving circuit. What I mean by tuning is this: if the receiving wire has the same period as the transmitting wire, then the wave coming from the transmitting wire will excite strong oscillations in the receiving wire under proper conditions; but if the receiving wire has not the same period as the transmitting wire, then the oscillations induced in it will be small. Now, even if the two are in unison, and the transmitting wire sends only a few rapidly decaying oscillations at stated long intervals, then you will get no appreciable resonance. To produce strong resonance you must send forth oscillations which have little damping. That has been shown experimentally in the case of the Hertzian oscillations, and these are, in a certain sense, like Hertzian oscillations, because they are of high frequency. Hertz showed that if you have a series of slowly decaying waves, and they strike a conductor having the same periodicity as the conductor from which these waves proceed, you then get very strong resonance effects Here is another method of producing strong resonance effects. If you produce a series of rapidly decaying waves, you will get small resonance, but this small resonance effect would be magnified if you could repeat these dampened impulses at very quick intervals. Instead of having eight sparks per second, suppose you had one hundred thousand sparks per second, each succeeding train of rapidly decaying waves will strengthen the effect of the preceding train. Then the current here in the tuned receiving circuit would swell up, and ultimately you will get a very large resonant oscillation by a sort of accumulative effect. So that in the present method of wireless telegraphy, we are laboring under two difficulties: one difficulty is that the waves proceeding from the transmitting wire are very much damped. The second difficulty is that the sparks do not proceed in a sufficiently rapid succession. Eight sparks per second—that is

nothing at all, it is the coarsest kind of dilettante work. It may be good enough for the present, but it is not enough for the possibilities of the future. If we are to speak about possibilities of the future, then it seems to me we ought to consider these two points: One is to produce electrical oscillation in this transmitting end, which are less damped; secondly, to produce a much more rapid succession of sparks, a much more rapid succession of impulses. Now, I am going to tell you one way that I think would operate and be successful in transmitting less dampened oscillations. I shall describe a method which I have tried, in connection with another scheme, for which there there are several claimants besides myself. Suppose we have a wire *a b c* G (Fig. 6) and a condenser *a c*. Suppose that the condenser plates have a shunt with a gap $a^1 c^1$. Connect the induction coil with the two sides of this spark-gap. This is a condenser having a considerably larger capacity than the capacity of the vertical wire, and it is preferable that the shunt wire have considerable self-inductance. This arrangement represents exactly the following mechanical analogy. In Fig. 7, A B C is a tuning fork, with its neck, C, rigidly fixed, and a string, A D, attached to it. One end, D, of the string is fastened. Suppose you strike this tuning fork, it will vibrate and make the string vibrate, and the vibration of the string will continue as long as the tuning fork vibrates. If the tuning fork is heavy, its vibrations will continue for a long time after each stroke The string performs forced vibrations, and not natural vibrations, which it would perform if it were fastened at its terminals and then struck. The forced oscillations of the string are not rapidly decaying, because the body vibrating it—the tuning fork—is a sonorous body. The condenser, *a c* (Fig. 6), with a shunt, is an electrical oscillator, an electrical tuning fork. The Hertzian oscillator is an electrical tuning fork. The analogy is perfect. It is not a mere superficial analogy; it is a perfect analogy. Every time you pass a spark, that is equivalent to giving the tuning fork a stroke. You start oscillations in this circuit just as you start the vibrations of the tuning fork by a stroke. These oscillations then keep up the oscillations in the vertical wire. Oscillations like that have been produced by Hertz himself. The wire was not connected to the earth, to be sure, but that does not make a very essential difference. I will give you an illustration of one Hertzian arrangement which is very interesting. It is represented in Fig. 8. A, is the induction coil, B C D E are four plates 16 in. square, and at a distance of 4 in., and *a b* is the spark-gap and E H and D K are two long wires with bridge, F G. The circuit, C B E F G D C, is the oscillator, the electrical tuning fork; the wires, E H and G K, are the electrical strings vibrated by the electrical tuning fork. This arrangement, the so-called Lecher arrangement adopted by Hertz, gives electrical oscillations, when the spark-gap is small, which are very sonorous.

After each spark a long series of waves will be emitted from a system like this. I don't care where these waves go, a part of them will certainly strike the receiving wire and will continue working there for some time. Now all you have to do at the receiving wire is to have another electrical tuning fork synchronized with that at the receiving end. Such an arrangement is given in Fig. 6. The condenser l with its shunt $o\ m\ n$ is the resonator. By varying the dimensions of the shunt $m\ n\ o$ or the capacity of condenser l we can tune this circuit. I do not see why such a circuit should not resonate. I would like some one to tell me why it should not. It does it in the ordinary conditions in the Hertzian experiments. I do not see why it should not do it here. There is nothing mysterious or even strange about these waves employed in wireless telegraphy, they being perfectly simple waves like any other electrical waves and can be made to obey the same rules. Now the receiving resonator will be even more sonorous, a great deal more sonorous than the transmitting oscillator. Two circuits may have the same period but different decrements. Two bells may sound the same note,

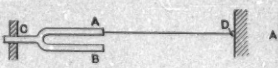

Fig. 7.

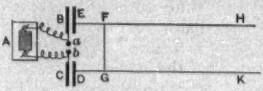

Fig. 8.

but one will continue for a long while to ring and the other may die out very shortly, depending on the internal friction and depending on the material from which the bell is made. The receiving resonator will have the same period as the transmitting oscillator when they are in unison, but the first one will have a larger decrement. That is because its frictional losses are larger. The coherer, f is, of course, supposed to be in a secondary winding as shown in Fig. 5. The oscillations once started in the receiving resonator will continue a much longer time than those in the transmitting oscillator. Now that is a very important point. Prof. Fessenden has told us that these oscillations are about three millions per second. I think he is very near the truth. They are considerably slower than the Hertzian oscillations on account of the length of the transmitting wire. Now these oscillations, excited by a resonator like the one in Fig. 6 are very persistent. Five thousand wave lengths, that is five thousand complete oscillations, will be emitted. Oscillations will take place before the resonator becomes exhausted. The oscillations in the oscillator are not so persistent on account of the air-gap. Say 500 oscillations take place after each spark before the oscillator is exhausted. That is, for five hundred three-millionths of a second, or for five thirty-thousandths or for one six-thousandth of a second,

these oscillations will persist after each spark, and then the electrical tuning fork is ready again to be struck again by an electrical blow, by a spark. You see that this oscillator is a tuning fork, that you cannot strike before the oscillations have subsided, because the air-gap conducts too well while the oscillations are going on. On account of the air-gap you have to wait until the oscillations die out before the induction coil can produce another spark. So that only after one six-thousandth of a second the oscillator will be ready to receive another spark. Now, the receiving resonator will continue considerably longer, there being much less damping. That is, after one six-thousandth part of a second, after the last wave-length is reached here the induced oscillations will continue for a considerably longer time, say ten times as long; that is, one six-thousandth of a second, plus ten six-thousandths of a second. This is of course very rough. I do not pretend to great accuracy. I only wish to make my meaning clear, and to illustrate it by a numerical example. One six-thousandth plus ten six-thousandths—well, make it twelve six-thousandths per second, which is one five-hundredth of a second. You see, the oscillations in the receiving resonator will continue for one five-hundredths of a second after the first spark in the transmitting oscillator. Now, suppose that you pass a spark every thousandth of a second. You see, then, that when the second spark takes place, and the wave-train sent out by this spark begins to arrive at the receiving end, the oscillations there have not died out yet by one five-hundredth of a second; there is still wave-energy left at the receiving end. Now, the second spark will add more wave-energy to the receiving resonator, the third still more, and so on. There will be an accumulative effect in the receiving resonator. It is just like having two tuning forks that are tuned to each other. Strike one tuning fork the second will resonate to some extent. Then, before the energy which the second tuning fork has received, has decayed, if you strike the first tuning fork again and again, the resonance of the second tuning fork will continually increase until it reaches a maximum effect, and you cannot go beyond that. That may be called the accumulated effect of resonance. You see, then, the utility of numerous sparks rapidly succeeding each other; I think a thousand per second is none too much. Well, the scheme would operate all right if you could do all these things. But here are the difficulties, and this brings us to the second point of my discussion. I have made a short calculation, using for the condenser capacity at the transmitting end about two-hundredths of a microfarad. That, is, the condenser plates are about a meter in length, and they are at a distance of one centimetre, and using a spark-gap of a quarter of an inch, requiring ten thousand volts, roughly. The coil will have to supply a kilowatt to the electrical tuning fork at the transmitting end, and a very large fraction of that kilowatt is used up in this spark-gap, because an elec-

tric radiator is not very much more efficient than an ordinary radiator; a great deal of it goes into heat. I think the efficiency is just about as bad, if not worse; so that perhaps 95% of that kilowatt would be used up in the spark-gap, and the rest is radiated into space, a very small fraction of which operates the receiving apparatus. What do you think becomes of the spark-gap spheres if the heat generated there is not carried off? They will be dirtier than the balls of Mr. Clark's instrument. I think that in less than no time they will be fused, and you won't be able to get any spark through them. There is the difficulty. The rest of that I leave for you to solve. We must put a great deal of energy into our transmitter in order to increase our distance of transmission. The more energy you put into any radiator, I don't care what it is, the better illumination you get. The more energy you can get into an electrical radiator, the more energy you can get out of it, the longer the distance over which you can transmit a certain amount of energy. There is not the slightest doubt about that. What we want, then, is, in the first place, to be able to put a great deal of energy into our radiators; secondly, very rapid succession of sparks; thirdly, radiators and receivers of small damping, and as a result of all these things, an efficient tuning of the receiving to the transmitting apparatus. The solution of these problems will increase the sphere of future possibilities of wireless telegraphy more than anything else that I know of.

THE PRESIDENT:—Is there any further discussion?

MR. CHARLES P. STEINMETZ:—I only want to add that I should not be afraid to dissipate even more kilowatts between a pair of balls. I think the best way would be turn a stream of high pressure air blast on it. You have no idea of the enormous amount of energy you can carry away in an ordinary air blast, twenty to thirty pounds pressure to the square inch. I had experience with that. I had occasion once to dissipate a considerable amount of energy between a pair of balls which I used in a high-frequency condenser charge.

MR. C. O. MAILLOUX:—I would like to ask Dr. Pupin if he would not be likely to have an additional difficulty in the synchronizing of his set of waves, which it seems to me would be essential for obtaining cumulative effects by resonance. When he starts a set of waves and gets resonance at the other end, the resonance can be kept up and increased only if the succeeding wave is in phase with the preceding one. It seems to me that we have to synchronize with the second set, and with all the sets succeeding each other, because otherwise it would be like vibrating a string, and then trying to vibrate it again in some different phase. Taking Dr. Pupin's example of the two tuning forks, if he struck the first tuning fork anew too soon or too late, or if he did not strike it at a certain critical time, or in the same phase of vibration, it seems to me the effect would be that the second fork

must synchronize anew, and hence I should think there would not be any cumulative effect in the resonance. Each wave would have only its own resonance.

DR. PUPIN:—No, there is no interference. Wave-energy is superpositive. The transmitting apparatus sends waves of the same period all the time. You can put them anywhere you please. There is no interference.

MR. MAILLOUX:—Do they accommodate themselves by mutual induction—the inductance effect or electrostatic effect?

DR. PUPIN:—Do you mean to say that when I sing ah, ah, ah, I synchronize the emission of my sounds, and that unless I do that you won't hear me? It is the same thing.

[Adjourned.]

DISCUSSION AT CHICAGO.

OPENED BY MR. ARTHUR V. ABBOTT.

MR. ABBOTT—Mr. Secretary and Gentlemen: If my understanding is correct, Mr. Marconi came over to this country to aid in reporting the recent yacht races, and it was expected that he would give the INSTITUTE in New York a description of the art that he has so successfully brought to a practical basis. Mr. Marconi had to leave this country sooner than was expected, and he was, therefore, unable to present his paper. We are, therefore, about to discuss this subject we have never listened to. Before, however, we can consider any topic with profit, it is desirable to know something about it; and I will take a little time to recall to you some principles probably seeming to many of you elementary, but which appear to me to be desirable to have prominently in mind.

All modern inventions and discoveries are based upon new applications in the utilization of energy. We do not find out new *things*, and we make very few new material combinations. But what the nineteenth century is remarkable for, is the discoveries, the inventions, the applications, the utilizations of the various forms of that which we now term energy.

It is but 200 years ago that heat was supposed to be a material substance. Newton thought that light was an essence; a something emitted by each shining body. And it is barely fifty years since Joule and Mayer demonstrated the mechanical equivalent of heat and work. So, one after another, sound, heat and light have been shown to be (as Prof. Tyndall so aptly described them), modes of motion, or forms of that something which we call energy, and which, in its essence, is as yet to us totally incomprehensible, totally unknown. All that we know of it is that in conjunction with matter, certain phenomena, certain results appear, that we term manifestations of energy.

At the beginning of the century, Franklin explained electricity on the hypothesis that it was material substance. He asserted that a positively electrified body contained more of a certain kind of fluid, and a negatively electrified body less. We can see how that idea yet remains, because at present we talk of currents and flows; we speak of electricity as running. But there isn't any flow; there isn't anything in the material sense of the word. Electricity is no more a substance than the light from an incandescent lamp. So we are now revising our ideas of electricity in exactly the same way that Young, and Joule, and Tyndall revised former notions regarding heat and light.

Twenty-five years ago Maxwell predicted that electricity would be found to be a kindred manifestation to the other then known forms of energy. We at the present time are not positive that electricity is a mode of motion. We are not quite sure of that. But I think that the evidence is strongly in favor of this view. On the other hand, some scientists speak of electricity as if it were identical with ether, stating that as it manifests properties similar to inertia, that it must be a substance. In reality this is only a talk about words. We never talk of heat as substance, but there is no doubt that heat is an ether manifestation. We never talk of light as a substance; but light is undoubtedly an ethereal manifestation. In each case the ether is the road, or means of communication whereby an energy manifestation taking place at one point may travel through space. But the ether is no more energy than the railway track is to the locomotive that runs over it. So, in the future the scientific world will probably regard electricity from the same standpoint that we now consider light, as etheric manifestations.

Maxwell, twenty-five years ago, with a mathematical insight that has never been equaled, gave certain equations, expressing in symbols all of the electrical phenomena with which we are now acquainted, and by means of which we are able to correlate electricity and light in such a way as almost to show that the quintuple group, light, heat, electricity, chemical action, and possibly the attraction of gravitation itself, is a vast chain in which each form is a link in the circle that brings every exhibition of energy back to its origin in motion.

Ten years ago Prof. Hertz, in Berlin—one of the most patient, one of the most careful, one of the most astute investigators that the world has ever seen, took up the problem of demonstrating experimentally some of the conclusions that Maxwell had shown algebraically.

Hertz's classic experiment, which I have assumed to be so familiar to you as to be unnecessary to present, consisted in taking two circles or rectangles of wire, one of which was connected to any source of electricity, such as a Holtz machine or an induction coil, and showing that when one circuit was properly related to

the other in its various electrical properties, an electrical disturbance set up in the first circuit would be followed by a corresponding electrical manifestation in the second circuit, even though the two were separated by a considerable space, thus proving that the electrification in the second was due to a wave action between the first and second, and that the second operated merely as a receiver. Hertz's discovery was a physical proof of the existence of the waves Maxwell had predicted, and as soon as Hertz's demonstration was placed before the scientific world it was immediately seen that if by any means it were possible to detect electric waves, it would be an easy matter to transmit intelligence from one point to another, over the highway of the luminiferous ether.

For if by any means we can excite electrical vibrations at one point, and can catch these vibrations at another point, it is simple to devise a code whereby the waves emitted at one point may be caused to convey intelligence to another. The synchronic circuits of Prof. Hertz were so insensitive in their ability to report the existence of electric waves, that it was impossible to show the presence of such oscillation more than a few feet away from the emitting apparatus. If I recollect rightly, Hertz' Memoirs do not describe any successful experiments over a distance greater than that occupied by a large room, say 50 or 100 feet. But Hertz also showed the possibility of reflecting these waves, or of refracting them, and of polarizing them, demonstrating that they followed and exactly corresponded to all that we know of all other forms of radiant energy.

In 1891 or 1892 Prof. Branley discovered that under certain circumstances a metallic powder become very sensitive to electrical radiation. He found that ordinarily a metallic powder, such as a heap of filings, offered a great resistance to the passage (now I have to go back to the old-fashioned word) of an electric current. If the two poles of a battery are dipped in metallic filings, no sensible current flows, as may be shown by a sensitive galvanometer. But if an electric wave, if the radiation from an induction coil or similar apparatus impinges upon these filings, a very curious change takes place, and the resistance formerly measurable in megohms, drops to a few hundred ohms or less. The filings offer, therefore, a means of detecting the presence of electrical radiation far more delicate than is yet found in any other way. They are the mechanical spectacles whereby the presence of electric waves can be made apparent to the eye of the brain. We are made acquainted with our environments solely through certain sets of nerves. Imagine a man deaf, blind and sensationless, what could he know about the world? Absolutely nothing. He could not hear, he could not see, he could not feel. Now through these three channels, and these only, we come into contact with the environments, and we translate to ourselves all the phenomena that take place around us,

and it is obvious that if there are any such phenomena which do not appeal to some one set of nerves, we are absolutely oblivious of it.

Standing on the top of a mountain, looking down into the valley below, one is insensible to what the eagle flying near one's head sees, because our eyes are not as acute as his. On the plains with the Indians, one does not perceive a thousand things that they instantly cognize. And why? Because our senses and our attention are not trained in the same direction as theirs. It is often said, that the modern eye is deteriorating. Here is the Indian; he can see a man five miles off. True, but the optician can make one see a man fifty miles off, because he can give a telescope; and on the whole I would rather have the telescope and my eye, than the Indian's eye without the telescope. A man can hear another shouting a few hundred feet away; but I would rather have a telephone and listen to New York. So we are constantly adding to, and increasing our perceptive limits, by means of mechanical appliances.

So the filings in the little tube, which has now been denominated a coherer, is a new mechanical appliance which enables us to penetrate into regions of space, which have previously been barred to us; which, so far as we are concerned, was empty. The magnetic waves from the sun at every eruption of hydrogen have radiated outward through space, enveloping the world and sweeping around it in the same way that billows of the sea engulf the grains of sand on the shore, and man has been utterly oblivious thereto; but every magnetic needle has trembled with every hydrogen eruption. The coherer is as yet insensitive to the radiation from the sun, but we certainly can detect that from the lightning flash. So Professor Brauley in that way has furnished us with another mechanical device for penetrating into the fields of radiant energy that surround us, in a way hitherto unknown.

Now I am going to request the lights to be lowered, so that we may consider more definitely what is known about radiant energy.

TABLE No. I.

Waves in the Air.

Sound Speed, 1,100 Ft. per Second.

	No 1. DESIGNATION.	No. 2. Wave-Length.	No. 3. No. of Vibrations per second
Eleven Octaves.	Visible Motion.....		*Less than 16
	Lowest Audible Sound.....	68 ft.	16
	Lowest Musical Note.....	34 "	32
	Man's Voice.....	8½ "	128
	Woman's Voice.....	4½ to 2½ ft.	256 to 512
	Highest Soprano.....	6 ins.	2000
	Highest Musical Note.....	3 ins.	4000
	Highest Audible Note.....	¼ in.	40000

The first table which I place upon the screen (See Table I) is a list showing us what the senses can gather from the slowest vibrations of an oscillating body. We are all pretty familiar with this form of wave motion, so I dwell on it but for a moment; just to call to your mind, that visible motion is something less than 16 per second. The lowest audible sound is about 16 per second, with a wave-length of about 68 feet; and the highest audible sound has a wave-length of a quarter of an inch, with about 40,000 vibrations per second. This table shows the group of waves which appeal to one set of nerves, the aural nerves; below 16 the ear refuses to recognize any vibration, and above 40,000 per second, the ear fails to hear.

TABLE II.

Waves in the Ether.

Heat and Light Speed, 186,000 Miles per Second.

	No. 1. DESIGNATION.	No. 2. Wave-Length.	No. 3. No. of Vibrations per second
Heat Waves 4 Octaves	Lowest Heat Sensation	583 millionths of an inch.	20 trillions
	Highest Heat Sensation.....	392 ten " " "	300 "
Light Waves 1½ Octaves	Red Light.....	271 " " " "	434 "
	Orange Light	258 " " " "	500 "
	Yellow Light	244 " " " "	520 "
	Green Light.....	207 " " " "	570 "
	Blue Light	191 " " " "	634 "
	Indigo.....	169 " " " "	680 "
	Violet Light.....	165 " " " "	740 "
	Ultra Violet Radiation.	140 to 85 ten millionths of an inch.	870 to 1500 trillion
	Roentgen Rays.....	Unknown.	Probably 300 quadrillions.

Now in Table No. II., we consider the next group of waves that the senses cognize. The ear can perceive vibrations from about 16 to 40,000; but from forty thousand up to twenty trillions per second there is no set of nerves that respond, and so far as we are concerned space is empty. But there is a large group of insects that probably have faculties, nervous systems, brain organizations, that enable them to recognize at least something of what takes place in that gap. Whether we shall ever develop any faculties of that kind is problematical, and so for the present at least, we must seek mechanical appliances to penetrate this region. From 20 trillions to 300 trillions the nerves in our skin, respond, and we may say that we feel a sensation that we call heat. With the sound waves we hear through a range of eleven octaves. Heat waves we feel only through about four octaves. The heat scale resembles the sound scale, and with the

proper instruments we may analyze this group of waves from 20 trillions to 300 trillions, and construct a thermal scale, corresponding to the audible scale.

Between 300 trillions, and 430 trillions, there is another gap at present, imperceptible to our senses, but at 434 trillions the eye takes up the analysis, and the retina translates to the brain the waves which impinge thereon, and we can see vibrations from 434 trillions to 740 trillions, giving a chromatic scale,—not chromatic in the musical sense, but chromatic in the color sense,— for the different rates of vibration are recognized as different colors. For there is absolutely no difference between a ray of red light and a ray of violet light, save in rapidity and in wave-length.

Beyond the end of the visible spectrum, photography shows us that radiation persists with ever-increasing rapidities, until we reach about 1500 trillions per second; then photography fails. But Professor Roentgen a few years ago showed us that the end was not yet, and there were still other fields, still other regions, in which vibratory energy increases in rapidity to an inconceivable extent; for when the mind attempts to picture a wave motion of 300 quadrillions per second—which we estimate to be the speed of the X-rays—one is in the presence of a number that is absolutely meaningless.

In the gap I spoke of a few minutes ago, between the upper audible limit and the lowest sensation limit, there is a space of about 31 octaves, and in this region the oscillation of electrical radiation upon which wireless telegraphy is based, takes place. This is the region into which we wish to penetrate—by means of the coherer; but we hope in the future to find something a great deal better, a great deal more sensitive than even that instrument. In Table III. I have collected some of the data pertaining to electric waves that may be of interest.

TABLE III.

Waves in the Ether.

Electro-Magnetic Waves, Speed 186,000 Miles per Second.

No. 1. DESIGNATION.	No. 2. Wave-Length.	No. 3. No. of Vibrations per second
Magnetic Waves from the Sun	1¼ millions miles	1 in 6½ seconds
Flash of Lightning	11000 miles	17
Induction Coil	18 miles	10000
Pint Leyden Jar	54 ft.	18 millions
Large Oscillator	1 ft.	100 "
Five Inch Oscillator	7 in.	192 "
Shortest Electrical Wave	3⅛ in.	480 "

Now, leaving the screen, we will see if I can excite some of the electric waves that are pictured in that list. Every electrical circuit has three properties—resistance, inductance and capacity.

The form of radiation obtained from any circuit depends entirely upon the mutual relations of these three electrical properties. Throwing a stone upon the surface of a smooth pond, excites a ring of waves that extends from the center of impact to the margin of the water.

Now, if we imagine the luminiferous ether—in which all of the known universe is supposed to be immersed—to be a spherical pond, and if we can imagine any means whereby we can throw stones into the ocean of ether and make splashes, we can obtain waves. This is exactly what we do in any electrical disturbance.

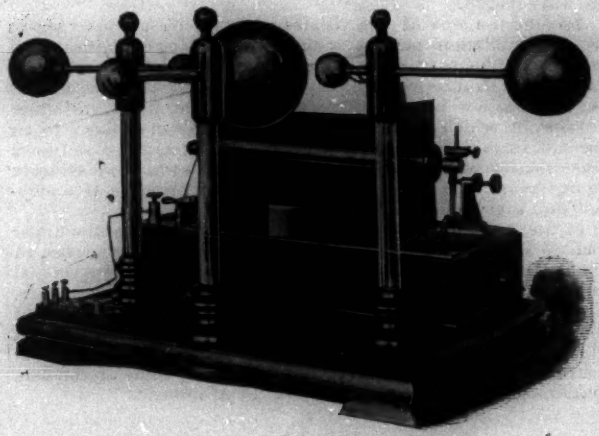

FIG. 9.—Spherical Oscillator and Coil.

On the table before us is an ordinary 6-inch induction coil. There is nothing remarkable about it, excepting that the terminals are furnished with brass spheres. If the coil is connected with a battery, by touching a key I can send a series of sparks between the termini. That coil is the electrical stone, and the spark is the splash in the ether, the concussion of which sets up oscillations that radiate outward in the form of an electrical wave. [See Fig. 9.]

Let us examine what happens in the case of the induction coil. Suppose there is an increased quantity of electricity on one pole of the coil, the positive, and a decreased quantity of electricity on the opposite side. Whether we regard electricity as a material substance, or if we regard it as ether, this hypo-

thesis remains equally true. If the electrical equilibrium be disturbed, there is a strong tendency to return to the original condition; and that tendency may be compared to action of the the attraction of gravitation upon a stone lifted above the surface of the earth. When the stone is on the surface of the earth, both are in equilibrium. But if we lift the stone, we have disturbed the earthly equilibrium; for each materially attracts the other. By the induction coil we disturb the ether, and that disturbance of equilibrium is followed by stress between the spheres, forming the poles that increased, until the resistance of the intervening space breaks down, and the equilibrium is restored with a crash.

But there is more than that, as the apparatus acts like a pendulum, for where the intervening space breaks down, the electricity, like the bob, swings from one pole to the other, back and forth several times, but with ever and ever decreasing intervals, as the stored energy is gradually dissipated, and the phenomenon ceases.

I say gradually; we talk of the electric spark being as quick as a flash. True, but a spark of this kind vibrates about 200 million times in a second; but 200 million times per second is a long time if compared with the Roentgen ray that vibrates 300 quadrillions per second.

The disturbance of electrical equilibrium, caused by a touch upon this key, gives rise to several series of waves, manifesting themselves in a number of different ways. First there is the sound of the spark; that consumes part of the energy. Second, one may feel, if the hand is near it, the heat of the spark; absorbing another portion. Third, the eyes perceive the light of the flash; abstracting more energy. And fourthly, by means of the coherer I will show in a moment, that there is still another form radiated through space to which the nervous system is absolutely insensible.

A coherer is an instrument consisting of a little glass tube, provided with two platinum-coated terminals, to which a battery may be attached, and having the space between the terminals filled with filings of metal. I will pass one around for examination. Exactly how the coherer works we do not precisely know; presumably something as follows: Suppose one is standing on the seashore, after a hot, sunshiny day, the sand is loosened, as one readily recognizes, by sinking ankle deep into it. The tide begins to rise, soon a higher wave sweeps over the sandy beach, and, falling back, leaves as a firm beach what a moment before was merely a loose mass of shifting quicksand. That is exactly what we suppose happens in a coherer. [See Fig. 10.]

In the center of this instrument is a pile of filings that are loose, as is the sand on a hot day. Now the electrical wave washes over that loose pile of metallic sand, and acting upon the ultimate molecules or atoms, it compacts them together.

Previously to the impact of the electrical wave the particles of the filings were not in electrical contact. They were separated by spaces; not very great actually, but great molecularly; separated by a sufficient space, so that the energy developed by two or three cells of battery could not cross the gap, and therefore the center of the tube was electrically open. When the electrical wave impinges upon those metallic particles, the gaps that previously existed are broken down, and the current from the battery is immediately established.

In this box are two or three cells of a dry battery, from which wires emerge and terminate in two brass supports, on which the coherer is placed. There is also a bell in the inside circuit. I will short-circuit the coherer. [Bell rings.] You see immediately the bell rings, showing that all that is necessary is to bridge the gap to cause the bell to sound. If I can excite electric waves by the coil that will wash those filings together, the bell ought to ring. [Bell rings.] But the metal-

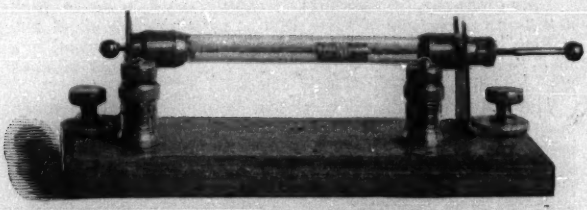

FIG. 10.—Coherer, Actual Size.

lic sand may be loosened, and should return to its former state of high resistance. Now I will dig the sand up again. I dig it up simply by hitting the tube, and the bell stops ringing. Of course this experiment may be repeated an indefinite number of times. It is simply a physical demonstration of the existence of the waves produced by means of the coil and of their detection by the filings which form the electrical eye, so to speak, that enables us to peer into that space which exists between the upper audible limit and the lower limit of sensation. [See Fig. 11.]

To come to some of the possibilities of wireless telegraphy. So far we have had the coherer in open space. What will be the effect of interposing other substance between the coil and coherer. We know that for thermal radiation, rock salt is transparent and glass is opaque; for luminous radiation, glass is transparent, and a brick wall is opaque; for Roentgen radiation a brick wall forms no obstacle. We can literally see through the masonry without the slightest difficulty. It is interesting to as-

certain what is opaque and what is transparent, to electrical radiation.

I will put the bell in a wooden box, and close it tightly. On touching the key the bell rings equally as promptly inside the box as it does outside. Wood is apparently transparent. Let us take another kind of box; here is one of metal. Let us see whether the metal is transparent. [Three trials and the bell fails to ring.] The metal is opaque.

Perhaps I can illustrate forcibly how a lightning rod works. I raise this vertical wire to catch the electrical radiation and bring it into the box, shutting it tight, and touch the key. [The bell rings.] In order to prevent the electrical wave from affecting the coherer we must screen it, not in an air-tight case; as of course this box is not air-tight;—but there must be perfect

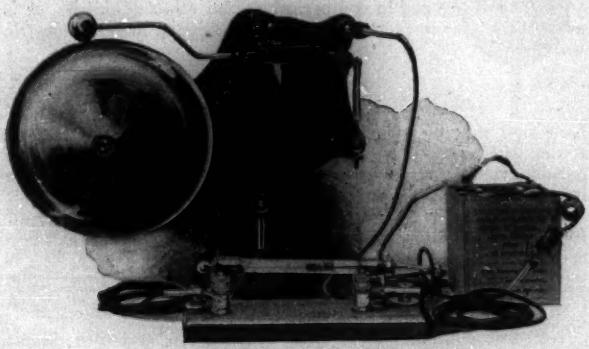

FIG. 11.—Simple Detector for Electric Wave.

metallic continuity around it. But when a wire was extended from the coherer through the case, a portion of the wave traveled down the wire, and influenced the filings. That is exactly the way a lightning rod acts.

I have shown so far, simply the way in which we can generate electrical waves, and the method by which it is possible to detect their presence. It may be interesting to go into the subject a little more technically. Denoting the capacity of a circuit by C, the inductance of the circuit by L and the resistance by R Lord Kelvin shows that if

$$R \text{ is } > \sqrt{\frac{4L}{C}}$$

the discharge of the condenser dies away regularly with the lapse of time;

$$\text{if } R < \sqrt{\frac{4\,L}{C}}$$

then the discharge gradually expends itself in a series of isochronous oscillations; the periodic time T of which is given by the following formula:

$$T = \frac{2\,\pi}{\sqrt{\frac{1}{L\,C} - \frac{R^2}{4\,L^2}}} *$$

If R is very small in comparison to the other quantities, as is usually the case in pieces of apparatus designed to emit electric waves.

$$T = 2\,\pi\,\sqrt{L\,C}$$

Now as T may be varied by changing either one, or both factors, a very wide range of vibration may be secured, and knowing the periodic time and rate of propagation of the wave, the wave length is easily found as the quotient of these quantities.

As L and C may be varied through an infinite number of forms, the oscillator, as any apparatus for emitting electric waves is called, may assume a corresponding variety of shapes, but as the familiar Leyden jar is always at hand, the evolution of the present most common spherical radiator is easy to trace, as shown in Fig. 12.

No. 1 is evidently a jar with a spark-gap and wire circuit more or less circular in form. In No. 2 the jar coatings have become a pair of plates separated by an air-gap. In No. 3 the circuit is straightened out and the jar coatings are a pair of disks, while in No. 4, the substitution of spheres for the disk is a natural transition to prevent brush discharge.

In Fig. 13 eight of the most common forms of oscillators are represented diagrammatically. No. 1 is a pair of straight rods terminating in knobs, separated by a short spark gap To increase capacity in No. 2, a pair of plates is added to the terminii of the rods. In No. 4 the rods are bent into a ring. No. 3 is a simple sphere, sparked to at either end of a diameter. No. 5 is composed of two equal spheres, the spark-gap placed on a line through their diameters. In No. 6 several spheres are used to produce proportionately greater intensity. While the spherical radiator is exceedingly convenient, it is so dead beat as only to

* Note.—See Lecons sur L' Electricite par E. Gerard, p. 256.

give two or three waves. Its vibrations may be prolonged at the sacrifice of intensity by enclosing it in a metal cylinder as shown in No. 7. A similar device is shown in No. 8, composed of a metal cylinder enclosing two spark knobs set on a diameter. For convenience, the formulæ for the wave-length of these forms are appended.

No 1—Two straight rods.

Let l = length of the rods over all, including spark-gap; d = the diameter of the rods, and D = the distance between their centers. Then

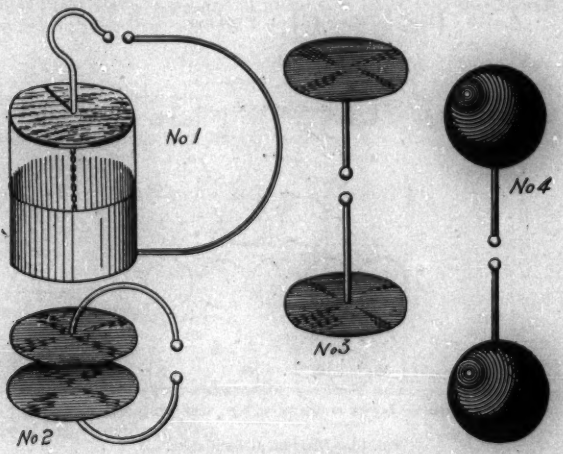

FIG. 12.—The Evolution of the Oscillator.

$$L = 2\,l\,(\log_e \frac{4\,l}{d} - .75.)$$

in electro-magnetic C. G. S. units.

$$C = \frac{l\,d}{4\,D\,v^2}$$

in electro-magnetic C. G. S. units, when v is the velocity of light.

$$T = 2\,\pi \sqrt{2\,l\left(\log_e \frac{4\,l}{d} - .75\right)\frac{l\,d}{4\,D\,v^2}}$$

If λ = the wave-length then $\lambda = v\ T$.

Approximately between the ranges of $l = 10$ cm. and $l = 200$ cm. and $d = .2$ cm. and $d = 5$ cm.

Approximately

$$\lambda = 15 \sqrt{l\,d}$$

In all cases l is supposed to be relatively large in respect to d.

No. 2—Rods with two equal square plates.

Let l = the distance between the plates including the spark-gap; d = the diameter of the rods = a = the side of one plate, then

$$L = 2\,l\left(\log_e \frac{4\,l}{d} - .75\right) + 2\,a\left(\log_e \frac{(1+a)^2}{(d/2)^2} + .5\right)$$

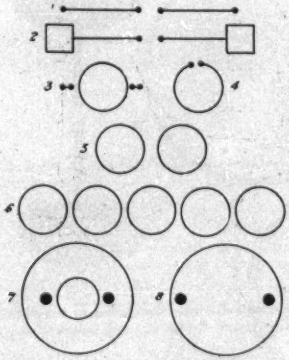

FIG. 13.—Diagram of Oscillators.

$$C = \frac{\frac{\pi\,d\,l}{2}}{2\,\pi\,l\,v^2} + \frac{a^2}{2\,\pi\,v^2\,(l+a)}$$

Multiplying and reducing, approximately

$$\lambda = 2\,\pi\sqrt{2.5\,d\,(l+a) + \frac{a^2\,(a+l)}{2\,\pi\,(a+l)}}$$

No. 3—Simple sphere diametrical spark-gap.

Let D = the diameter of the sphere, l = the sum of the length of both conductors, on each side of the sphere which form the spark-gap; and d the diameter of these conductors; then

$$L = 2\,l\,\left(\log_e \frac{4\,l}{d} - .75\right)$$

$$C = \frac{D}{2}$$

$$\lambda = 2\,\pi\,\sqrt{D\,l\left(\log_e \frac{4\,l}{d} - .75\right)}$$

If l and d are very small in proportion to D, then this reduces approximately to

$$\lambda = 1.3\ D.$$

No. 4—A plane ring.

Let R = the radius of the ring and d = the diameter of the rod of which it is made; then

$$L = 4\,\pi\,R\,\left(\log_e \frac{8\,R}{d} - 2.\right)$$

$$C = \frac{\pi\,d}{2}$$

and approximately

$$\lambda\ 4\,\pi^2\,\sqrt{2\,R\,d}$$

No. 5.—Two equal spheres.

Let D = the diameter of the spheres and b = the distance between their centers; then

$$L = 2\ D\,\left(\frac{1^2}{D^2/4} + .5\right)$$

$$C = \frac{R}{2}$$

and approximately

$$\lambda = 7\ D.$$

The remainder of the shapes of oscillators given are merely modifications of the preceding cases.

As electric oscillations only accompany a disruptive discharge, the more sudden and violent the spark, the greater is the proportion of energy converted into radiation, therefore, the adjustment of the spark-gap is of vital importance in the construction

of an oscillator. All parts of the apparatus should be carefully rounded to prevent any approach to brush discharge, and the surfaces of the gap should have as large a radius as possible. These surfaces must also be maintained in the highest state of polish, particularly if short waves are desired, for even a few minutes' sparking will so roughen the metal as to prevent the emission of the shortest oscillation. To minimize the labor of re-polishing it is convenient to make the terminals of adjustible cups in which polished spheres may be placed, continually revolved, so as to present fresh surfaces for the emission of sparks. The immersion of the terminals in oil has also been tried with good success, as it largely contributes to the efficiency, permanency and suddenness of the spark action. The length of the spark is another important and very essential consideration, but one which requires adjustment for every different arrangement of oscillator and receiver. A gap either too long or too short will fail. Usually from one-half centimetre to three centimetres

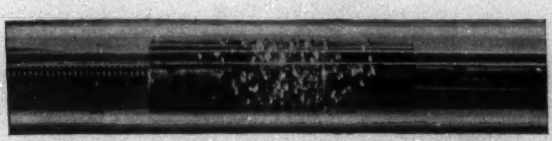

FIG. 14.—Coherer Magnified.

in length is fairly successful. The observer soon learns to judge from the character of the spark as to the working of the oscillator. A thin faintly luminous violet spark gives poor results and indicates too small capacity in the apparatus. A thick, heavy, perfectly straight white spark is equally bad, denoting an excess of capacity, while the most efficient spark is jagged and arched and plays about the discharge surface. It appears to have a reddish violet center and to be surrounded with several concentric sheaths of flame. With each reversal of the coil current, the spark is suddenly extinguished with a loud snapping sound, and is also very sensitive to a magnet.

That is a formula that you can practically use. From those equations it is practical, within reasonable limits, to design a circuit which shall give you any rate of vibration that you wish.

Now let us take our next slide.

I will put upon the screen some representations of the coherer. This first slide is a magnification of the coherer which I passed around a few minutes ago. You will see the brass plugs at either end coated with platinum, and the metallic filings in the center. [See Fig. 14.]

In the next slide I have an arrangement which I think will illustrate the way in which the coherer works, and it may be interesting to know that it is taken under the microscope from the filings themselves.

Under ordinary circumstances the coherer filings are in the position that you now see, electrically opaque. An electric wave arrives and jostles the atoms into contact, and the coherer becomes electrically transparent. A slight tap destroys the atomic bridges, and again becomes electrically dark, as soon as the next electrical wave appears, it becomes electrically transparent again, and so on; so that the action of the coherer, under a succession of waves, may be illustrated by the operation of this slide on the screen.

I have endeavored, thus roughly, to describe the way in which we make electric splashes in the ether, and the means for diluting the waves. Imagine a blind man standing on the margin of a pond; he might hear the splash of a stone that was thrown into the water, but he would have no means of knowing anything about the waves which ripple over the surface. Suppose a piece of board placed on surface of the water, and the blind man to put his hand thereon, the oscillations of the chip, under the influence of the waves, would convey the idea that "Somebody has thrown in a stone."

Without the coherer we have no senses which can detect the electrical waves. We cannot see them, hear them, feel them, taste them; they flow by us, and have for ages, but the coherer is the board that enables us, by mechanical means, to detect their presence. If we can appreciate the presence of these waves, then we can use them for the transmission of intelligence.

We are all familiar with the Morse alphabet. If we can send waves for the tenth of a second and stop, and then send waves for three-tenths of a second, we have what is equivalent to a dot and a dash, and symbols A. If we send waves for three-tenths of a second and then stop, and then for one-tenth, and stop, and then for one-tenth, and stop, we have D. If we devise means whereby the coherer shall stop transmitting the battery current as soon as the waves stop, it is evident that we can measure the length of time that the waves are passing through the ether. To do this manually by tapping, is far too clumsy, and some automatic means must be employed. One method is to place in the coherer circuit an electro-magnet. As soon as the battery current is established the electro magnet is excited, and—if we use the buzzer form arranging the hammer so it shall strike the glass tube, it will tap the coherer and decohere it. That is exactly what happens in this instrument on the table. Here is a coherer similar to that which you have seen with the tube supported by springs at each side. Directly underneath is the buzzer, an ordinary electric bell. The electric waves emitted by the coil will excite the coherer; operating the buzzer and decohering the instrument. Let us

try the coil. [Circuit made, bell rings but does not stop.] The coherer is adjusted a little too sensitively. I will try and modify it. [Breaks tube.] I am afraid this instrument will not operate any more to-night, because I have broken the tube, and I shall have to have recourse to what my professor of natural philosophy used to tell his students when his experiments did not succeed. "The principle, gentlemen, remains the same, although the experiment failed." In this case, however, the experiment did not quite fail. It worked at least half way until I endeavored to adjust the tube, and broke the coherer. So that I am sorry that I cannot give you an illustration of the actual sending of a mes-

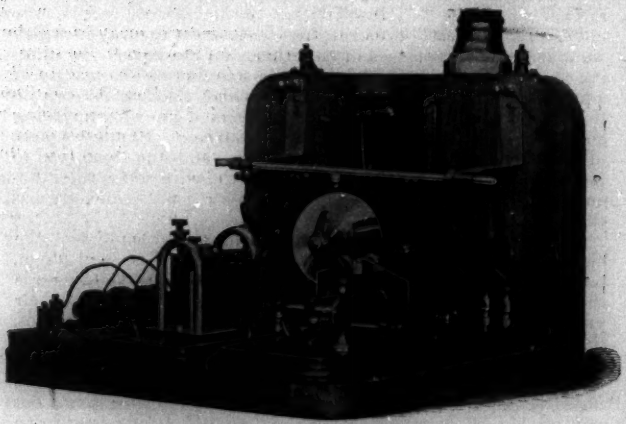

FIG. 15.—Receiver with Electro-Magnetic Decoherer.

sage across the room, as I had expected to do this evening. [See Fig. 15.]

However, the other apparatus which I have is entirely typical. At the transmitter, there is the induction coil, and a wire rod extending into the air terminating in a brass ball, while the other pole of the coil is grounded. At the receiving station, the coherer is in series with a sensitive relay, the local of which carries a buzzer or hammer, mechanical hammer and a sounder. This is supplied with a similar elevated mast and sphere, for while the other terminal is grounded, exciting the transmitter sends a spherical undulation through the ether, which, falling upon the coherer, causes the filing to cohere, completing the circuit that energizes the relay, and operates the sounder, sets the buzzer running, and mechanically de-

coheres the filings, leaving them ready to receive the next wave.

I will now place upon the screen the circuit which I use, and then touch for a moment on the limitations of the apparatus. [Slides are shown). As bearing upon the commercial value of this method, an inquiry into the efficiency and delicacy of the coherer is interesting. The common six-inch coil takes a current of ten or twelve amperes at a pressure of ten volts, thus absorbing about 120 watts. The efficiency of the coil is not over 30 to 50 per cent.; using the latter amount as most favorable to the coherer, about 50 or 60 watts would re-appear in the spark-gap, the heat, light and sound of which absorb at least 90 per cent. of the energy there expended, thus leaving not over ten per cent. or say five or six watts as transformed into pulsations

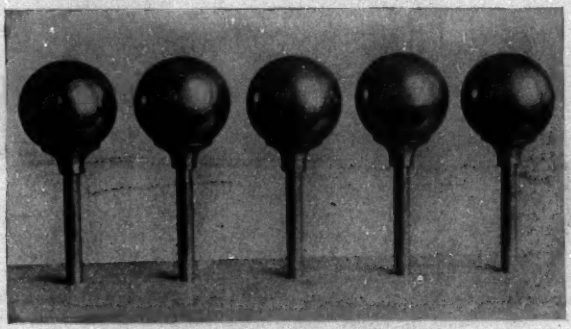

FIG. 16.—Multiple Oscillator.

through the ether. Suppose the coherer to be located at a distance of 50 feet from the coil. The area of the spherical wave is then $100^2 \times 3.14 = 31{,}450$ sq. ft, or 4,500,000 sq. inches. A coherer without wings, exposes a surface of less than a square inch to the electrical oscillations, hence the apparatus can only absorb an amount of energy of about one seven hundred thousandth of a watt, and calculating back to the energy delivered to the coil, the efficiency of about one eight hundred thousandth of one per cent is found. By the use of a receiving plate and the most delicate apparatus, signals have been sent commercially in England over a distance of 35 miles. Merely for the purpose of illustration, suppose, under such circumstances, the coil to consume 1,000 watts and deliver 100 watts as radiated energy, and assume that the receiving station is supplied with a plate of 100 sq. ft. Probably all these quantities are in excess of those act-

ually employed, so that the calculated results will be favorable to the coherer rather than against it. Under such circumstances, the spherical wave will be

$$70^2 \times 3.14 \times 5.280^2 = 420{,}000{,}000{,}000 \text{ (approximately).}$$

and the energy delivered per square foot of surface will be

$$\frac{100}{420{,}000{,}000{,}000}$$

The amount received by the coherer will be

$$\frac{100 + 100}{490{,}000{,}000{,}000} = \frac{1}{42{,}000{,}000}$$

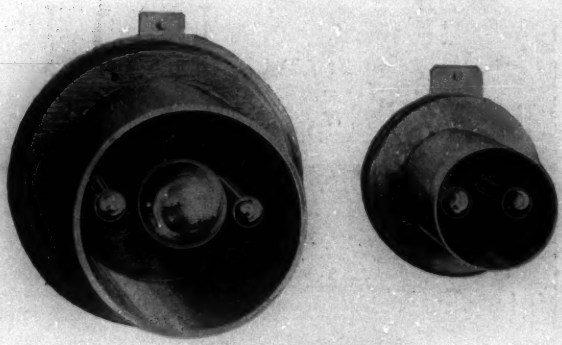

FIG. 17.—Dead Beat Oscillator.

say one fifty millionth of a watt, and the efficiency one five hundred millionth of a per cent. While such low efficiency is abhorrent to the commercial instinct of the calculating engineer, one stands amazed at the delicacy of the apparatus that will so swiftly and unerringly detect such minute quantities of energy as are represented by the amounts of the order of a millionth of a watt.

We see, therefore, that while the instrument is marvellously sensitive, its efficiency is equally marvellously low.

As an illustration, if a man were up in the top of this building and wanted to talk to a man in the street, he could put his head out of the window and shout. He would undoubtedly get his correspondent, and equally attract the attention of everybody in the neighborhood. If only one message is to be sent, it prob-

ably would pay to shout; but if one wants to talk from roof to basement frequently a speaking tube is supplied. Why? When the unaided voice is used, the vocal organs send out spherical sound waves that spread in every direction; but with the speaking tube the sound waves are confined and made to travel along a predetermined route, and the correspondent will receive the whole of the energy except that which is dissipated in heat in the tube.

If there is one message to be sent between the points, it may be a very good plan to use an oscillator and coherer; but if one wants to send a million messages, it is better not to waste so much energy; better build a road for the energy to travel on; and so far the best electrical road is a wire, or to use Lord

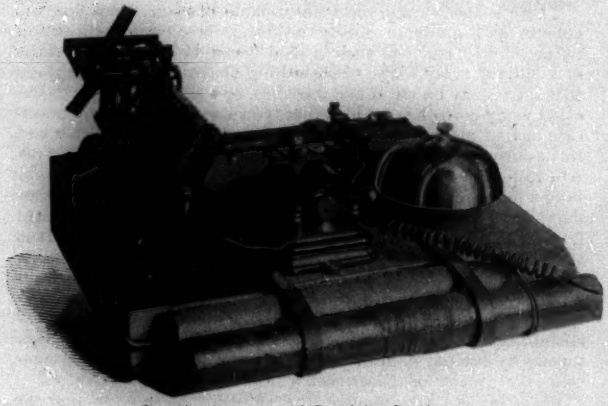

FIG. 18.—Receiver with Revolving Decoherer.

Kelvin's apt phrase, the hole in the dielectric through which electricity runs. It is much cheaper to build a *hole* than to try to fill the whole of space. So where a number of messages are to be transmited, the cheaper plan is to build a line. Such questions are always finally reduced to considerations of expense, and the engineer must always, in deciding, calculate what the expense will be, by both methods, and select the cheaper.

If messages are to be sent between two lighthouses, or between a lighthouse and the shore on a rocky coast, where a cable is expensive and must be frequently removed, wireless telegraphy may be cheaper than a cable. If one is to signal between ships at sea, where a cable is impossible, wireless communication solves the problem. Between two moving army corps, where a line can-

not be maintained, wireless telegraphy is excellent; but for the transmission of newspaper matter between New York and Chicago every day in the year, it is commercially absurd.

Then there are other limitations. It is remarked that wireless telegraphy could not be secret; anyone with a coherer can read the ether waves. Yes; so anybody can read inductively any known form of telegraphy. There isn't a secret telegraph line in the world. Inductively I can tap any telegraph or telephone line, and be undetected. But one can always use a code, and then the man who taps lines, is no wiser than before, unless he is the equal of the cryptogram decipherer in the "Gold Bug." So by this expedient wireless telegraphy, or any other kind of telegraphy, may be just as secret as you wish.

There is another and more formidable obstacle. Assume the coherer and oscillator nicely adjusted, both stations working harmoniously, and you wish to send messages between war ships or a moving army corps, or something of that kind, and suppose that the enemy on an other war ship, or the commander of the other army corps has an idea that you are about to communicate in that manner; he simply sets up another oscillator and rattles away at his key in any incoherent fashion, your coherer will respond to all waves, and the jumble that results would take a more fertile imagination than that of Edgar Allen Poe to decipher. So wireless telegraphy at the present is an egotistical performance; only one man can have the field at one time. It is stated that it is possible to so synchronize one instrument with another that those two will mutually respond, and be oblivious to all others. This is partially true. A certain amount of synchronism can be established, and instruments made not to respond markedly to those that are much out of tune, but this possibility has not been developed to even an experimental success over greater distances than a few hundred feet as yet.

So, as far as I have studied the subject, the matter may be thus briefly summed up: As a scientific invention, as an instrument of marvelous delicacy, the coherer will go down to the ages as one of the most wonderful instruments man has ever invented. For certain specific cases, under special circumstances, signaling with its aid is of immense value; but at present, and unless there is some improvement, some modification that does not as yet appear, its use will be confined to such special cases.

MR. GEO. M. MAYER:—I would like to ask Mr. Abbott if there has been any attempt to use reflectors or similar means.

MR. ABBOTT:—Yes Prof. Hertz showed that it is possible to reflect electric waves, and Mr. Marconi has made experimental attempts to use them. Electric waves are susceptible of reflection, refraction and polarization, but the practical difficulty is in the construction of the apparatus. Waves, such as we are using, vibrate about 200 millions per second, and the wave-length varies from a few inches to several hundred. Now imagine the

difficulty of building a reflector large enough to embrace such waves: or the possibility of grinding its surface so accurately that the wave would not be distorted perceptibly in projecting it 100 miles, what would the reflector cost? The Yerkes lens cost something like $50,000, and it is forty inches in diameter. What would the probable expense of a lens for electric wave be? I don't mean to say, probably not proportionately as great, but for transmission over the same distance, for commercial work, a wire is cheaper.

MR. THOMAS DUNCAN:—I would like to ask, Mr. Abbott, whether anybody has succeeded in getting the wave-length down to two and a half inches. Hertz, I believe did.

MR. ABBOTT:—The shortest wave-lengths that have been recorded, are by Dr. Lodge, and are three to four inches long. You remember I showed you that the length of the wave was equal to three-tenths times the diameter of the sphere used on the oscillator. By using a sphere small enough, we can reduce the wave-length. The electric waves that we are using now, have a rate of about 200-millions per second. Light varies from 450 to 730 trillions per second. Mathematically, if a sphere was used small enough, one could get electric waves as short and quick as light waves. Then we would have light without heat, but the dimensions of a sphere sufficiently small are about what we suppose to be the dimensions of the atoms; and therefore from our present knowledge, light is an oscillatory vibration of atomic electric charges, and if at any time we can succeed in devising any means for vibrating the electric charges of atoms, we shall have cold light, and we shall not have to spend ninety-five per cent. of our coal energy in getting five per cent. of light out of incandescent lamps.

MR. JAMES LYMAN:—I would like to ask if the coherers cannot be made of different sensibilities, so that it would be possible to have a set of coherers operating on our vessels that will not be sensitive to the users of other oscillators that might be set up with the object of preventing signaling, or with the object of setting up other signals.

MR. ABBOTT:—Yes, and no, in this sense; that the sensitiveness of the coherer is a function of three things; it varies as the capacity of the pressure upon the filings, and the chemical condidition of the metal. It is conceivable that you can make a coherer which will be sensitive to one set of waves and insensitive to another set, because a different amount of energy is delivered to the different coherer.

As far as the construction of the coherer is concerned, it may be interesting to note that the sizing of the particles must be carefully done. They must be very sharp and of non oxidizable metal. Gold, silver and platinum make much more sensitive coherers than the oxidizable metals.

MR. V. R. LANSINGH:—What is the present limit of satisfactory working of the system?

MR. ABBOTT:—They claim in England a distance of 90 miles. The longest actual distance that I have known of its being successfully worked is something under 70 miles.

MR. LANSINGH:—How rapidly can the key be worked?

MR. ABBOTT:—You can readily see that this sort of apparatus must be worked very slowly. The best speed that has been attained is about eighteen words a minute. The average transmission is about six to eight words per minute.

[Adjourned.]